disk would be obtained, which could be placed exactly in the plane of the sun-aperture. The glass should be attached to the micrometer-frame, and thus no disturbance of the images by any influence of a support could occur.

If the illumination were made by magnesian or electric light, this apparatus would be also available for Photographic experiments on the Transit.

On the Increase of Probable Errors in a Transit of Venus as dependent upon the Smallness of Normal Velocity. By E. J. Stone, Esq.

In the *Monthly Notices* for April there appears a short paper of mine, "On the Comparative Clinging of the Limbs of *Venus* and the Sun in the Transit of 1874," and some consequences which appeared to follow therefrom.

In discussing the observations made in 1769, I convinced myself that the chief source of error to be feared was erroneous assumption of semidiameter. The errors arising from causes which chiefly affect ordinary transit-observations, such as errors in subdividing seconds of time, loss of time in mechanically registering impressions conveyed through the eye to the mind, &c. sink into insignificance compared with the three or four seconds which we had to allow for the probable error of a contact observation.

In observations of this kind the observers have to seize some well-marked phases of the gradual destruction of, and restoration of, the irradiation near the point of contact of the Sun's limb. The appearances are complicated by considerations of atmospheric disturbances, aperture of telescope employed, density of absorbing medium made use of to reduce the Sun's glare, eye of the observer, defining character of the telescope, and the power employed. But, under given conditions, the phenomenon — as, for instance, real contact or total disappearance of connecting ligament — will take place with a definite angular separation between the limbs.

If the attention is drawn to the first appearance of, or disappearance of, the connecting ligament, then that ligament must have a definite breadth, in order that, under the given circumstances of observation, it may be visible by contrast upon the Sun's disk. The question of seeing the ligament is not a question of sufficient time to enable us to pick it up, but of sufficient optical means to distinguish it. Such, at least, was and is my own opinion.

In the transit of *Mercury*, 1868, November, Mr. Carpenter, observing at Greenwich with a small telescope and low power, about 90, states that "the black ligament appeared to form *in-*

stantly, and to be of the breadth indicated, about $\frac{1}{3}$ the diameter of the planet." Now, although Mr. Carpenter observed the first formation, to him, of the black ligament, his time is about 13^s later than the time given by Mr. Lynn, observing with a power of about 170, and with a much better instrument. There was here plenty of time for Mr. Carpenter to see the ligament within the 13^s which elapsed between Mr. Lynn's observation and his own, but he did not see it, because, I maintain, his instrumental means were insufficient to appreciate the existence of the ligament by contrast, until it had become of a certain definite breadth. If this be the case, he would not have seen the ligament sensibly sooner had a period of 24^s elapsed whilst *Mercury* was moving over the distance from its position corresponding to Mr. Lynn's observations to that which it had at the time of his own observations.

The errors of such observations really arise when we assume that the phase of the phenomenon observed by one observer corresponds to the phase observed by another observer in such a way that each takes place at the same distance between the centres. The error thus committed is measured by the deviation of the true distance between the centres at which the observation was really made, and the mean distance for all the observations of apparently similar phenomena which are to be combined. The errors, therefore, measured in time, must be inversely proportional to the normal velocities. Another illustration, with a practical bearing, may be given, as follows:—Suppose that photography should be applied to the transit of *Venus*, by obtaining a great many photographs, by some quick process, whilst *Venus* is on and near the limbs, at points as near the points of maximum effect as possible. Then, with such a process, the errors in measuring the angular distances between the centres will be sensibly independent of the relative motions of *Venus* and the Sun; but if we should wish to apply these results to the determination of solar parallax by referring back all the photographs to some definite distance from the Sun's centre for comparison with photographs made at other stations, then, in referring these results back and determining the *time* at which *Venus* was at some definite distance from the Sun's centre, we should commit errors in *time* in proportion to the slowness of normal velocity.

These views led me to infer that the probable error of every single observation made in 1874 must be considered as injuriously affected by the slow normal velocity, and I gave in my April note the ratio of the mean probable errors arising from this cause in the transits of 1874 and 1882. I can understand differences of opinion existing with respect to the accuracy of these views; but I must confess my inability to find any full consideration of such views brought forward in the March number of the *Monthly Notices*.